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## **Propaganda vs. education : a case study of hate radio in Rwanda**

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Propaganda vs. Education: A Case Study of Hate Radio in Rwanda

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## **Propaganda vs. Education: A Case Study of Hate Radio in Rwanda**

### **Abstract and Keywords**

This chapter discusses whether education limits or exacerbates the effects of state-sponsored propaganda on political violence. It provides evidence for the hypothesis that basic education can limit the effectiveness of propaganda by increasing access to alternative media sources. It builds on the case study of the Rwandan genocide in work done by Yanagizawa-Drott (2011), and shows that the propaganda disseminated by the “hate radio” station RTLM did not affect participation in violence in villages where education levels, as measured by literacy rates, were relatively high. A discussion of the potential underlying mechanisms driving the results is presented. The methodological challenges of identifying causal effects of mass media and propaganda are also described, including recent innovations using statistical methods that may be used to overcome those challenges.

**Keywords:** Media effects, education, literacy, radio, political violence, Rwandan genocide

is a lack of basic education a necessary condition for propaganda to induce civilian mass violence? The 1994 Rwandan genocide was an event unique in the speed of killings and widespread participation of ordinary citizens in carrying out the massacre. Many believe that propaganda and inflammatory “hate media” calling for the extermination of the Tutsi ethnic minority played a significant role in fueling the violence (Smith 2003; Thompson 2007). In Yanagizawa-Drott (2011, hereafter YD), I found evidence supporting the hypothesis that mass media can induce mass violence. I estimated that approximately 10 percent of participation in the Rwandan genocide can be attributed to propaganda spread by the radio station Radio Télévision Libre des Mille Collines (RTLM). However, the conditions under which propaganda does and does not induce individuals to participate in mass violence are not well understood. In this chapter, I discuss the role of education and literacy in the efficacy of radio-based propaganda aimed at inducing mass violence.<sup>1</sup>

There are two competing views regarding the relationship between education and political persuasion. One is that education limits the persuasive effects of propaganda (Haider-Markel and Joslyn 2009; Zaller 1992). According to this hypothesis, education raises political awareness by exposing individuals to alternative information sources and fosters the cognitive resources to critically evaluate the validity of any given mass media source. An alternative view is that propaganda and education are complementary. That is, the effects of state-sponsored propaganda can be exacerbated if schooling instills obedience to authority, indoctrination, and acceptance of the ideology of the elites (Gramsci 1971; Kremer and Sarychev 2000; Lott 1990, 1999).<sup>2</sup> If education fosters a particular political ideology or opinion, individuals may more easily assimilate propaganda when its content is consistent with the political ideology of the education system (p. 379) (Lord, Ross, and Lepper, 1979). These two competing views are plausible on theoretical grounds. Ultimately, however, whether education exacerbates or limits the effects of being exposed to propaganda in any given context, such as the 1994 Rwandan genocide, is an empirical question.

Quantitative analysis of the effects of mass media is by no means new. Following two highly influential studies by Paul Lazarsfeld and co-authors (Lazarsfeld, Berelson, and Gaudet 1944; Lazarsfeld and McPhee 1954), the political communications literature has traditionally focused on political behavior in terms of voting and opinion. Although this literature initially concluded that mass media seemed to have “minimal effects” (Klapper 1960), recent studies have identified a stronger relationship (e.g., Iyengar and Simon 2000).<sup>3</sup> However, there is little systematic evidence as to whether mass media intended to cause mass violence can induce participation in violence, and it requires investigation to determine how literate populations respond to radio propaganda differently from illiterate ones.<sup>4</sup>

This chapter consists of three parts. First, I provide a background to the Rwandan genocide and discuss how education—in particular, literacy— may have played a role during this episode of violence. Second, using Rwanda as a case study, I also discuss the fundamental methodological challenges in using statistical analysis to test for the outlined hypotheses. In many contexts, the supply of propaganda is not random but rather *targeted* toward certain audiences. I investigate the data from Rwanda and provide evidence suggesting that this was the case with RTLM. When propaganda is targeted, I argue that statistical

analysis is unlikely to correctly uncover the causal relationship between exposure and behavior, unless the researcher can find variation in exposure to the propaganda that is unrelated to preexisting political views and behavior. I also describe how recent innovations by applied economists have been able to overcome these challenges, and how they were used in YD.<sup>5</sup>

Finally, I use data from YD to shed light on the relationship between propaganda and education. YD presents evidence suggesting that the effects of RTLM on participation in the genocide were weaker among more educated people. I take the analysis a step further, to show that, in areas where the literacy level of the population was relatively high, there were no effects of RTLM broadcasts on violence. Based on the data from Rwanda, YD and this chapter together therefore provide evidence suggesting that a necessary condition for radio propaganda to induce civilian mass violence is that the targeted population lacks basic education.

## The Rwandan Genocide: RTLM Propaganda and Education

The Rwandan genocide occurred from April to July 1994. During this short period, an extermination campaign led by Rwandan public officials and *Interahamwe* militia (p. 380) groups resulted in approximately 800,000 civilian deaths. About three-quarters of the victims were from the minority Tutsi ethnic group. Compared to the relative size of the ethnic groups, this implies a killing rate five times that of the Holocaust (Caplan, Markusen, and Melvern 2003). In addition to killings by organized militias and the army, a high proportion of the killings were carried out by hundreds of thousands of Hutu civilians, the ethnic majority in Rwanda. Most of these Hutu civilians involved in the genocide used simple weapons such as machetes and clubs (Verwimp 2006). The genocide ended once the Rwandan Patriotic Front (RPF), a rebel group consisting of Tutsi refugees based in Uganda, defeated the Rwandan army and seized control of the country in July 1994. Given that civilians performed such a large portion of the violence, it is of particular interest to investigate the role of propaganda in motivating participation in the genocidal violence.

Ethnic polarization, as well as rhetoric of hatred between ethnic groups, was long established in Rwanda prior to the genocide (Kimani 2007). These divisions were deepened by anti-Tutsi propaganda spread by the newspaper *Kangura* during the period 1990–1993. In 1993, RTLM, often referred to as the “hate radio” station, was founded. This station partly drew its language and inspiration from existing arguments made by *Kangura* and others in the years leading up to the genocide (Kimani 2007).

Founded by ethnic Hutu extremists ten months before the genocide, RTLM explicitly called for the extermination of the Tutsi ethnic group. Radio broadcasts motivated the violence as

a necessary preemptive self-defense to prevent Tutsi political domination. For example, on December 2, 1993, the RTLM broadcast the following message:

Tutsi are nomads and invaders who came to Rwanda in search of pasture, but because they are so cunning and malicious, the Tutsi managed to stay and rule. If you allow the Tutsi-Hamites to come back, they will not only rule you in Rwanda, but will also extend their power throughout the Great Lakes Region.

(Mironko 2007; author's translation from Kinyarwanda)

In addition to appealing to the need for Hutu dominance, the RTLM targeted Hutu racism against the Tutsi and often called them “cockroaches” (Chrétien 2007). The task of killing Tutsi was referred to under the euphemism of “work” (Li 2007).

How may the RTLM broadcasts have affected beliefs and behavior? Propaganda never works in isolation, and its persuasive power is arguably context-dependent. Its impact on a person's beliefs can generally be said to depend on three key intervening and contextual factors: *prior knowledge and beliefs*, *exposure to information*, and *trustworthiness of the source* (Shiraeve and Sobel 2006).

How is this framework relevant for the case of RTLM during the Rwandan genocide? First, the trustworthiness refers to the credibility of the radio station. This credibility was greatly bolstered by government backing of RTLM. In fact, RTLM was financed and controlled by a small faction of Hutu insiders within the government, called the *Akazu* (Caplan 2007). This group, widely known as “le Clan de Madame,” had at its core the president's wife, family, and close associates. In fact, the propaganda was viewed as an important tool by the Hutu extremist government; in a broadcast, (p. 381) Prime Minister Jean Kambanda called the station and, on air, proclaimed it to be “one of the key weapons in the war” (Kimani 2007). This pronouncement left no doubts about whether the government supported the RTLM broadcasts. In addition to the trustworthiness lent to RTLM through government support, the station was popular due to its modern and informal style of broadcasting. RTLM aired popular songs, and listeners could call in to request songs or to chat with the witty broadcasters (Des Forges 2007).

The second factor, exposure to information, can be represented by access to the RTLM broadcasts.<sup>6</sup> In YD, I used data on RTLM reception to estimate its impact on violence. Access depended primarily on signal strength within a village and accessibility to a radio. In the following sections, I will describe how these datasets were created and detail the statistical method used to assess how exposure to RTLM information affected behavior during the genocide.

Third, the effects of exposure to RTLM may depend on one's prior beliefs or knowledge. Prior beliefs and knowledge can be viewed as the result of multiple underlying information processes, including formal education, peer interactions, and access to alternative news sources. In Rwanda, a large set of newspapers spanning the political spectrum provided the main alternatives to RTLM. On the brink of the genocide, approximately forty newspapers in the country could broadly be placed into three categories, based on their political affiliations: (1) pro-government newspapers, (2) political opposition newspapers,

and (3) pro-RPF newspapers (Higiro 2007).<sup>7</sup> The pro-government newspapers were affiliated with the National Republican Movement for Democracy and Development (MRND), the ruling political party of President Habyarimana. Several of these newspapers also supported the far-right Hutu extremist party Coalition for the Defence of the Republic (CDR). Of the eleven pro-government newspapers, *Kangura* was the most extreme in its anti-Tutsi propaganda. It repeatedly published articles dehumanizing Tutsis and their leaders, and consistently depicted them as the enemy.

There were about twenty political opposition newspapers, associated with either the Democratic Republican Movement (MDR) or the Social Democratic Party (SDR). President Habyarimana's political base was in the north. The political base for these two parties—MDR and SDR—was in the southern part of the country, where previous President Gregoire Kayibanda (an ethnic Hutu) had been born. The editors of the opposition newspapers also came from the south and explicitly supported an alternative political agenda, compared to the pro-government media sources. According to Higiro (2007), two of the opposition newspapers (*Ikindi* and *Isibo*) received financial assistance from the United States embassy.

There were seven pro-RPF newspapers in the country. Their founders and editors were ethnic Tutsis. The newspapers were highly critical of MRND and CDR, and generally provided information about the political agenda of the RPF and its military successes. The pro-RPF and the political opposition newspapers therefore provided stark alternatives to the pro-government media sources and RTLM in terms of information provision and what political agenda was promoted.

(p. 382) Unfortunately, due to lack of available data on circulation, it is not known how many readers each newspaper had. However, based on anecdotal evidence, Higiro (2007) estimates that the circulation for a newspaper was about two thousand to three thousand, where the price for a newspaper in Kigali was 100 francs (\$0.75). In rural areas, it was also common for political organizations to distribute their newspapers free of charge to local leaders and supporters.

The high rate of illiteracy—nearly half of the total Rwandan population—was a major obstacle to the distribution of these newspapers, particularly outside of Kigali (Des Forges 1999; Higiro 2007). Thus, these illiterate people were limited to what they heard from neighbors and from the radio, thereby limiting their access to alternative sources of news. In this sense, education enabled individuals to access information that was different from the government-backed propaganda. The efficacy of the RTLM broadcasts in inducing violence may therefore have been limited among the more educated parts of population.

However, the education system also fostered the political ideology of the Hutu regime (McLean-Hilker 2011). As this ideology was similar to that pushed by the RTLM broadcasts, more educated people may have interpreted the propaganda differently. In particular, empirical evidence in psychology shows that individuals may suffer from *biased assimilation*, which is the tendency to accept information that confirms one's prior belief (Lord, Ross, and Lepper 1979). Thus, if education in Rwanda affected one's prior beliefs or obedience to authority, the RTLM broadcasts may have been most effective in inducing

violence among relatively educated individuals. A priori, it is unclear what mechanisms will be present or dominate. Thus, the question of whether education in general, and literacy in particular, exacerbates or limits the potential effects of propaganda is best settled by empirical means.

## A Look at the Data

To measure violence in YD, I used a dataset on prosecution rates for violent crimes committed during the genocide. This prosecution data came from local-level *Gacaca* courts across all of Rwanda. Set up in 2001, the *Gacaca* court system was created to process the hundreds of thousands accused of committing crimes during the genocide.

For each village, I obtained data on the number of prosecutions for violent crimes committed during the genocide. There are two legal crime categories: crimes committed by leaders and organizers of the genocide, and crimes committed by civilians. This chapter focuses solely on prosecutions for civilian violence.<sup>8</sup> These prosecutions include (1) authors, co-authors, or accomplices of homicides or serious attacks that caused someone's death; (2) those who intended to kill and caused injuries or committed other serious violence, but without actually causing death; and (3) those who committed criminal acts.

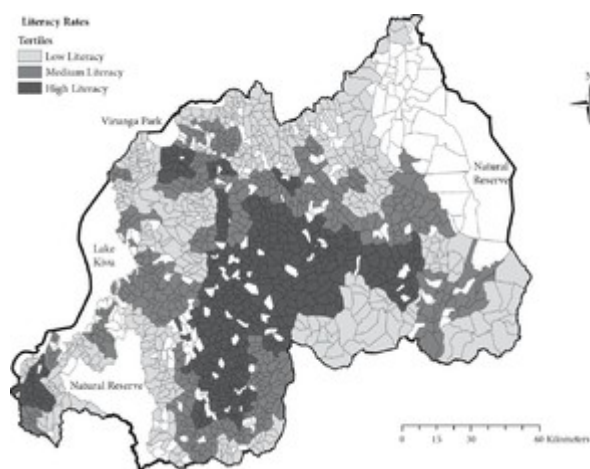
(p. 383) In the sample used for analysis, there were approximately 382,000 cases of civilian violence. Additional datasets were used to construct controls variables. Literacy rates were taken from the 1991 Rwandan census. Because only a handful of villages had zero prosecution cases, the data indicate that almost all villages experienced violence during the genocide. Literacy rates among the Hutu population in a village were also obtained from the 1991 census. Figure 20.1 shows the distribution of literacy rates across the country, divided by tertiles.

To measure exposure to propaganda, I constructed a village-level dataset on RTL radio reception. For each village, radio coverage was calculated as the share of the village area with coverage. The idea is simple: for a given fraction of households with radio, higher radio coverage indicates a greater likelihood of the villagers listening to the broadcasts.



# Basic Correlations

To investigate whether access to RTLM broadcasts resulted in increased participation in violence, and the role of literacy in that participation, Table 20.1 presents simple correlations among radio reception, participation, and a set of audience-related variables. The first row of Table 20.1 shows the correlation coefficient between RTLM radio coverage and the participation rate to be -0.09. At first glance, this correlation shows that villages with good radio reception experienced *less* genocidal violence, on average, compared to villages with little or no radio reception. Does this mean that the RTLM broadcasts *caused* fewer people to participate in the violence? That seems implausible. To identify the true relationship between the access to broadcasts and violence, we must first examine the characteristics of the villages with access to the radio broadcasts compared to those villages without access.



Click to view larger  
Figure 20.1 The Hutu literacy rate in villages in Rwanda. Low literacy means that the literacy rate is below 48 percent (bottom tertile), whereas high literacy means it is above 52 percent (top tertile). White areas indicate missing data.

The placement of the transmitters determined which villages could and could not listen to the broadcasts. One of the two transmitters was placed in Kigali, the capital, and (p. 384) (p. 385) the other transmitter was placed on a high mountain in the northwestern part of the country.

Table 20.1 Correlation coefficients of RTLM radio coverage

Violence	
Persons prosecuted for civilian violence	-0.09
Audience	
Fraction of households that are Hutu, 1991	0.25

Population density, 1991	0.21
--------------------------	------

Fraction of households with radio	0.24
-----------------------------------	------

### **Prior Knowledge**

Literacy rate, Hutus, 1991	0.18
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*Note:* A unit is a village.

A closer look at the data shows that the transmitters were by no means placed randomly. First, Table 20.1 shows the correlation between the fraction of Hutus in a village and RTLM radio coverage. This correlation is positive, implying that the greater the Hutu majority in a village, the more likely that village had RTLM radio coverage. In addition, Table 20.1 shows that the population density in 1991 was positively correlated with RTLM radio coverage. More densely populated villages had greater access to RTLM radio broadcasts. Finally, there is also a positive correlation between the fraction of households in a village owning a radio and RTLM radio coverage.

These data on radio coverage provide support to the seemingly obvious strategy that the RTLM *targeted* the Hutu population as an audience. A large fraction of the Hutu population lived in the northern part of the country. Furthermore, the northern part of the country was different from the south in important political aspects. Both the origins and political base for the *Akazu*, which provided financing for the RTLM, were located in the north and northwestern part of Rwanda.

Ethnic tensions were also particularly strong in the northern part of the country because of a civil war leading up to the genocide. This civil war began in 1990, when the Tutsi rebels, known as the Rwandan Patriotic Front (RPF), launched attacks from Uganda into northern Rwanda. After setbacks, the RPF conducted guerrilla warfare in northern Rwanda from its bases in the northwestern border areas (Melvern 2000). RTLM focused on the threat from the RPF as a key reason for ethnic Hutus to kill ethnic Tutsis. In particular, the station emphasized how progress made since independence in 1959 was at risk unless the Hutus fought back (Li 2007). Thus, the fact that the second transmitter was placed in the northern part of the country provides further support that this was done to maximize listening rates among the audience most receptive to RTLM's messages.

Regardless of the motive, it is clear that access to RTLM was not random. The correlations show that, before the genocide, villages that were able to listen to RTLM broadcasts (during the genocide) were very different from villages that could not listen to the

broadcasts. Villages with access to the broadcasts had a high proportion of Hutus, high population density, high levels of radio ownership, and a higher rate of literate people. The simple correlation between radio coverage and violence is therefore unlikely to capture the causal effects of exposure to the propaganda.

## How Can We Identify the Causal Effects of Propaganda?

In this section, I discuss how the statistical framework originally developed by Rubin (1974, 1977) can be used to define the causal effects of propaganda.<sup>9</sup>

(p. 386) The logic goes as follows. Suppose any given village (or individual) can potentially receive or not receive the RTLM propaganda through radio broadcasts. The causal question of interest is whether participation in violence in a given village is affected by the RTLM propaganda. To learn this, we need to imagine the *counterfactual outcomes*. That is, what would have happened in a village that received the propaganda had the village never received it? Therefore, for any given village, there are two potential participation rates: one rate if propaganda is received and one rate if propaganda is not. The difference between the two is the causal *treatment effect* of receiving propaganda.

Furthermore, different villages may have different treatment effects. That is, in some villages, the conditions are such that propaganda might not have had any effects, whereas in other villages, the effects might have been large. For example, for villages with illiterate populations and low newspaper circulation, the treatment effect could be large relative to villages with high literacy rates.

Ideally, one would want to know the treatment effect for each village. Obtaining these data is not feasible, however, because each village either does or does not receive the propaganda. Since we cannot observe the counterfactual outcome for each village, we must learn about the effects of propaganda by comparing the average participation rates in villages that did receive the broadcasts to the average participation rates in villages that did not. The more humble goal, which is feasible, then, is to assess the *average treatment effect* of propaganda.

Table 20.1 shows that villages with good reception typically had lower rates of civilian participation in the violence, compared to villages with little or no reception. But we also know that villages with good reception were very different to begin with: the Hutu majority was relatively large in these areas, the population density was higher, and the villagers were generally more literate. Since these factors most likely affect violence directly, the simple correlation reflects a *selection bias*. The negative correlation therefore reflects two

underlying effects—the true average treatment effect of propaganda and the selection bias.

What is the selection bias in the case of RTLM propaganda during the Rwandan genocide? It is possible to make educated guesses. By definition, the selection bias is the difference between two averages. In particular, it is the average of the participation rate in the *absence* of propaganda in the villages that *did* receive it, minus the average of the participation rate in the villages that did not receive it.<sup>10</sup> Since the first term is a counterfactual outcome that must be estimated—the level of violence that would have happened in the villages that received propaganda had they never received it—the selection bias can never be known with certainty.

In many cases, one can reason what the sign of the selection bias might be. To see this, I will examine two sources of selection bias: relative sizes of each ethnic group, and proximity to the RPF rebels.

First, an important dimension to understanding local violence during the genocide was the relative sizes of the two main ethnic groups. When the Rwandan genocide ended, only about 25 percent of Tutsi minority was left alive. In many villages, the entire Tutsi population was killed. Therefore, the grim reality is that, in villages where (p. 387) there were few Tutsis, violence against Tutsis was inherently limited. A lower share of the Hutu population was able to participate in the killings. Thus, villages that ended up having good reception would have had lower participation rates, *even in the absence of propaganda*. By this logic, the selection bias is likely negative.

A second source of selection bias is the geography of villages with access to the broadcasts. The genocide finally ended in July 1994, after the Tutsi RPF rebels seized control of the country. Advancing from positions in the north (Prunier 1995), the RPF systematically prohibited attacks on ethnic Tutsis. Therefore, the violence took place over a longer period in some villages compared to others. This provides a key explanation for why the violence, in the form of participation, was substantially lower in some areas of the north. Because of the *proximity to RPF rebels*, many villages in the north would have had lower participation rates *even in the absence* of RTLM propaganda. This selection bias is also negative. It is also likely to be large. This means that the simple negative correlation between radio reception and participation rates in Table 20.1 does not reflect the causal treatment effect of the broadcast.

In addition, Table 20.1 shows that the broadcasts were targeted at villages with higher literacy rates. If literacy and education affect the propensity to participate in political violence, this is an additional source of selection bias. If education makes individuals less likely to participate in violence, perhaps because education increases ethnic tolerance or makes the opportunity cost of conflict higher, then the selection bias is negative. Analogously, if education increases the propensity to join the genocidal violence, for example because education instills obedience to authority, the bias is positive.<sup>11</sup>

The important lesson is that, even if the true causal effect of RTLM is positive (so that propaganda leads to more violence, not less), the negative correlation between radio coverage and participation in Table 20.1 may simply reflect a true effect being masked by a negative selection bias. Thus, to capture the causal effect, it is necessary to eliminate the selection bias.

## Eliminating the Bias

As a benchmark, it is useful to think about the methodological ideal. From a strictly scientific point of view, the ideal method to test for propaganda effects would be to conduct a randomized experiment in which villages (or individuals) are randomly assigned to either receive propaganda or not. Randomizing eliminates the selection bias because the comparison villages (i.e., the villages that do not receive propaganda) then mimic the counterfactuals of the villages that do receive the propaganda. Since the only systematic difference between the two groups of villages is whether or not they receive propaganda, they should be similar on *all* other dimensions that affect violence (e.g., education, poverty, population density, ethnic tolerance, etc.), and the difference in the average participation rates will capture the average causal treatment effect of propaganda.

(p. 388) In many contexts, setting up an experiment that randomly assigns treatment is not feasible. This is especially true for propaganda intended to induce hatred or violence, since it would be grossly unethical to do so.<sup>12</sup> In these cases, other nonexperimental methods employing historical observational data must be used.

The most common strategy to address selection bias has traditionally been a *selection-on-observables* approach using multiple regression analysis. In this approach, the selection bias is eliminated by statistically “controlling for” all other factors that both are correlated with treatment (propaganda, in our case) and affect the outcome of interest (violence). In the Rwandan case, such factors would include ethnicity, population density, RPF rebel presence, education levels, and the like.

Sometimes, the problem cannot be solved, as the data are not available. As mentioned above, radio reception was most likely better in areas with close proximity to the RPF rebels, meaning that reception and distance to the RPF rebels are correlated. We should therefore control for distance to the RPF rebels. However, there are no available data on the exact RPF positions at the brink of the genocide. Therefore, we cannot control for the variable, even though this is necessary to eliminate the selection bias.

But how do we know with certainty what all the “other factors” are? The answer is that, in most cases, we don’t. Theory can provide a guide, but ultimately it is an empirical question. A fundamental problem with the selection-on-observables approach is, therefore, that the researcher does not know with certainty what factors to control for.<sup>13</sup>

What should the researcher do in this case? If he decides to go ahead and run the regression analysis anyway, using data that are available to him, controlling for size of the Hutu population but omitting (intentionally or unintentionally) distance to the RPF rebels, then the bias has not been eliminated. Since a key variable that must be included was omitted, the estimates are then said to suffer from *omitted variable bias*. Analogous to the selection bias logic described earlier, this bias means that the correlation between radio reception and participation in violence still cannot be interpreted as causal. In the end, little is learned about the effect of propaganda from this regression analysis.

## A Method for Estimating the Effects of Broadcasts

The key to eliminating the selection bias is to find an empirical strategy that approximates randomized experiments and where the assignment mechanism of propaganda is *as good as randomly assigned*.<sup>14</sup>

In 2009, Olken investigated how watching television impacts social capital in Indonesian villages.<sup>15</sup> First, by using algorithms that predict electromagnetic propagation, Olken was able to predict reception across different areas in Indonesia. Importantly, by using a technique exploiting topography, he obtains “as good as random” assignment of access to the broadcasts.

(p. 389) In a mountainous or hilly region, a household’s ability to access certain television channels depends on the signal strength, and thus the topography in and around their village affects reception. Radio reception will partly depend on whether hills happen to lie in the line of sight between the transmitter and the village. Some villages might have a large hill in the line of sight and therefore will be unable to receive the broadcasts, whereas others with a clear line of sight will get a strong signal. By isolating this source of variation in reception, reception can be said to be as good as randomly assigned, giving rise to a natural experiment.

This methodological approach is suitable for estimating radio or television effects as long as there is sufficient variation in topography. Known as “The Land of the Thousand Hills,” Rwanda is largely made up of hills and valleys. Employing this strategy therefore has the potential to solve the problem of selection bias.

One can partly assess whether access to broadcasts are “as good as randomly assigned” by testing whether access is associated with other determinants of participation in violence. Villages with and without radio reception should be similar in terms of pre-genocide ethnic composition, population density, education levels, and so on. If the villages were systematically different, then selection bias could still be a problem.

When I employed this empirical strategy in YD, I found that villages with good RTLM radio reception appear to be no different in terms of pre-genocide characteristics than those without good reception, including population density, population levels, distance to the nearest major town, and distance to the nearest major road. This set of “placebo” tests therefore confirms the assumption of access to broadcasts being “as good as random.” This approach, in turn, allowed me to estimate the causal effects of RTLM propaganda on violence during the genocide.

## Did RTLM Have an Impact on Violence in High-Literacy Villages?

Using the empirical strategy outlined above, in YD, I showed that access to RTLM broadcasts increased participation in the genocide, on average. The research also provided evidence that the effects are weaker in areas with higher education levels among Hutu households. In line with this logic, one can test whether there are any effects at all in areas where the population was highly literate.

To do this, I restrict the analysis to villages belonging to the upper tertile in the Hutu literacy rate distribution. That is, I include the one-third of the villages (a subsample of 351 villages) in Rwanda that have the highest literacy rates among Hutu households. Table 20.2 presents the results.

Comparing villages within administrative communes and a set of control variables, Column 1 shows that the effect of RTLM is very close to zero. The coefficient implies that complete radio coverage increased violence by 2 percent. However, the result is (p. 390) not statistically significant. Column 2 adds additional control variables. The coefficient is negative, small in magnitude, and not statistically significant. Hence, there is no evidence that RTLM increased violence in the villages with a high literacy rate among Hutu households.

Table 20.2 The effect of RTLM in villages with high literacy rates

	Civilian violence: Log(prosecuted persons)	
	(1)	(2)
RTLM radio coverage	0.02 (0.49)	-0.13 (0.49)
Population in 1991, log	0.79	0.85

	<b>Civilian violence: Log(prosecuted persons)</b>	
	(0.24)**	(0.24)**
Observations	351	351
R-squared	0.41	0.45
Additional controls	No	Yes

The sample consists of a subsample of 351 Rwanda villages with a literacy rate above 52 percent (top tertile) among Hutu household heads. All regressions include commune fixed effects and radio propagation controls (latitude, longitude, and second-order polynomials in village mean altitude, village altitude variance, distance to the nearest RTLM transmitter). Additional controls are population density in 1991, distance to major town (log), distance to major road (log), distance to border (log), slope of village dummies (north, east, west). Standard errors in parentheses, clustered at the commune level. For a data description, see Yanagizawa-Drott (2011).

(\*\*\*) Significant at 1 percent,

\*\* Significant at 5 percent,

\* Significant at 10 percent.

Compared to the main results in YD, what can explain the differential effects on violence? One explanation has to do with prior knowledge and access to alternative information sources. Radio Rwanda was the government's radio station, and there was no television. Thus, newspapers were the main alternative to RTLM. However, in many rural areas, newspaper circulation was low, due mainly to low levels of literacy (Higiro 2007). Since the newspapers from the political opposition parties and the pro-RPF newspapers provided alternative viewpoints, the information environment was arguably richer in high-literacy villages. Therefore, one explanation is that these literate individuals may have been more critical of the political message that extermination of the Tutsi population was necessary for Hutu survival.

An alternative explanation is that educated individuals are also economically better off, and that the subsample therefore includes only relatively well-off villages. If relatively wealthy individuals are less prone to violence, or less susceptible to propaganda, this consideration could explain the pattern. However, YD shows that the effects of RTLM were larger in villages with wealthier Hutus. This factor is therefore unlikely to be the main explanation.



Could differential exposure to RTLM explain the results? There are no data on listening rates, so this is not feasible to investigate directly. However, this is unlikely to be the (p. 391) main explanation. First, radio coverage was 38 percent *higher* in high-literacy villages. In addition, radio ownership was 29 percent higher in villages with high literacy. Prima facie, it therefore seems unlikely that differential exposure to the RTLM broadcasts provides the main explanation.

## Conclusion

This chapter used the Rwandan genocide as a case study to investigate the role of literacy on the impact of radio propaganda. In particular, the chapter provided insights into the differential extents to which highly literate compared to less-literate populations were induced to participate in the genocidal violence. The chapter also described a statistical framework that can be used to estimate effects of radio and television in other contexts.

Extending the analysis in Yanagizawa-Drott (2011), I show that the RTLM broadcasts did not have any effects in high-literacy villages. The radio propaganda thus seemed to be ineffective among the literate population. Based on the data from Rwanda, the results therefore provide suggestive evidence that a necessary condition for radio propaganda to induce civilian participation in violence is that the targeted population lacks basic education. When alternative print media provides competing views and information, as was the case in Rwanda in 1994, a plausible explanation for why basic education may limit the persuasive power of a given propaganda outlet is that it enables citizens to access alternative news sources. However, more studies are needed to identify the specific conditions and contexts under which propaganda can affect violence and ethnic conflict.

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## Notes:

- (1) . Propaganda can broadly be defined as “the spreading of ideas, information, or rumor for the purpose of helping or injuring an institution, a cause, or a person” (from Merriam-Webster dictionary).
- (2) . Recent experimental evidence from Kenya by Friedman et al. (2011) provides support for this hypothesis.
- (3) . This is likely due, at least in part, to methodological differences and the use of randomized field experiments (see Green and Gerber 2008).
- (4) . For a qualitative analysis of hate radio during the Rwandan genocide, see Straus (2007). For a study of how media and education affect political beliefs, see Gentzkow and Shapiro (2004).
- (5) . For readers familiar with modern applied micro econometrics, this section can preferably be skipped.
- (6) . Exposure to information can include both unbiased and biased information. In this chapter, I make no assumption as to whether the process by which individuals combine prior knowledge with new information is rational (i.e., Bayesian) or biased in different ways.
- (7) . There were also about ten newspapers published by the Rwandan Tutsi diaspora in different countries, such as *Impuruza* in the United States, that were clandestinely circulated in Rwanda.
- (8) . Civilian violence is individual violence that is not carried out by the army or paramilitary organizations. Yanagizawa-Drott (2011) also uses data on more organized forms of collective violence.
- (9) . It is worth noting that this is different from so-called Granger causality. For a discussion of how Rubin’s model relates to earlier notions of causality by philosophers, see Holland (1986). For an excellent book on how to use statistical methods to estimate causal effects, see Angrist and Pischke (2008).
- (10) . Strictly speaking, it is the differences in the average of the participation rate in an infinitely large sample of villages when the propaganda is fixed.

(11) . Krueger and Maleckova (2003) provide evidence of education increasing the propensity for political violence. They find that Hezbollah fighters are more likely to be literate and have secondary education, compared to the general Lebanese population.

(12) . The opposite case is feasible, however. Paluck and Green (2009) ran a field experiment in Rwanda investigating the impact of radio programs promoting intergroup tolerance.

(13) . At some level, if this were known, there would not be much reason to conduct research on the factors causing violence and conflict in the first place.

(14) . This includes, for example, quasi-experiments, natural experiments, instrumental variables, and regression discontinuity. For discussions of natural and quasi-experiments, see Meyer (1995) and DiNardo (2008). For methods of causal inference using observational data, see, for example, Angrist and Pischke (2008, 2010).

(15) . Olken (2009) finds that increased signal strength led people to spend more time watching television and was associated with decreased participation in social organizations and lower self-reported trust.

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